AERODYNAMICS.

ELI W. BLAKE.

ORIGINAL SOLUTIONS

OF

SEVERAL PROBLEMS

IN

AERODYNAMICS.

By ELI W. BLAKE.

NEW HAVEN:
TUTTLE, MOREHOUSE & TAYLOR, PRINTERS
1882.

PREFACE.

The employment of the laws and principles of dynamics in discussing the movements of the parts of an aerial medium caused by the action of a local force impressed upon them, or by their own inherent elastic force causing them to flow towards a total or partial vacuum, has heretofore been thought by mathematicians to be attended with great difficulties; so great indeed that few attempts were made to surmount them, and none that were attended with full and complete success. Hence many problems in aerodynamics, that were of great interest to science and the arts, remained unsolved. Some years since I became deeply interested in an attempt to solve one of these problems, and after much thought succeeded in devising a method whereby a satisfactory solution was accomplished. Perceiving that the same method, with suitable modifications, was applicable to the solution of other problems, I gave my thoughts to others from time to time as I found leisure amid the active pursuits of life; and the results of these investigations, as they were respectively reached, were published in the then current numbers of the *American Journal of Science.*

Believing that the methods employed in those investigations might be employed with success in solving other important problems, and desiring to bring this field of research to the attention of physicists, I recently prepared another paper pertaining to this branch of physics and offered it for publication in the *Journal of Science;* but the editors, finding in it positions and conclusions which conflicted with their pre-conceived opinions; and not being disposed to publish that which they were not ready to endorse, declined the article.

Under these circumstances it was decided to print the paper, prefixing to it the articles that had been published in the *Journal of Science* pertaining to aerodynamics, arranged in the order of their respective dates; and in this collective, but somewhat disjointed form, to present them as a contribution toward a more full development of this interesting and important branch of Physics.

ARTICLE I.

*A Theoretical Determination of the Law which governs the Flow of Elastic Fluids through Orifices.**

The subject announced at the head of this article, is not only interesting considered simply as a subject of scientific inquiry, but it is also a matter of practical importance in its relations to several branches of mechanism. Among these may be instanced, as perhaps first in importance, the bearing of the subject upon the construction of the steam engine. The size of the pipes and valves which conduct the steam to and from the working cylinder, should be properly adjusted to the size and velocity of the piston. In general, the larger these pipes and valves the better, so far as respects the power of the engine. But there are inconveniences attendant on making them large; and in order to make a due compromise between the inconvenience that may be incurred on the one hand and the amount of power that may be sacrificed on the other, it becomes necessary to understand correctly the law which governs the flow of elastic fluids through orifices. Treatises on the dynamics of fluids have not omitted to give rules for the determination of such questions; but it will be seen in the course of this article that those rules are very defective.

If the velocity with which a fluid flows through an orifice from one vessel into another be represented by V, the density under which it passes the orifice by D, and the area of the orifice by S, then the product VDS is the measure of the quantity of fluid discharged in a given time. It is an established law in the dynamics of fluids, that the velocity of the flow is directly as the

* Published A. D. 1848, in the American Journal of Science, second series, vol. v, page 78.

square root of the pressure and inversely as the square root of the density. If, then, the efficient pressure which produces this flow be represented by P, the general law expressed by symbols will be,

$$VDS \propto \frac{DS\sqrt{P}}{\sqrt{D}}.$$

The above expression is in accordance with the received theory, and properly understood it is correct and applicable to all fluids, elastic as well as inelastic. But it must be observed that D in this expression must in all cases represent the *density under which the fluid passes the orifice.*

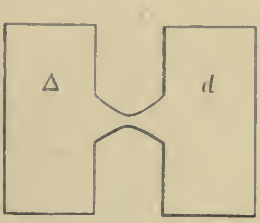
In all the treatises on the dynamics of fluids that I have examined, the quantity D in the foregoing expression represents the *density of the fluid in the discharging vessel:* it being assumed that the fiuid passes the orifice without change of density. This assumption is correct so far as respects inelastic fluids, but as respects elastic fluids it is far otherwise. A particle cannot even *begin* to approach the orifice without a change of density. Surrounded by other particles, it will not begin to move until the pressure before it becomes less than the pressure behind it. If the pressure before it is less than the pressure behind it, then the density there is less also, and consequently the density of the particle itself is diminished, for that must be intermediate between the density before and behind it; and as it cannot begin to move without a change of density, so for the same reasons its motion cannot be accelerated without a further change of density. Thus for every increment of its velocity in its approach to the orifice, there must be a corresponding decrement of its density. Hence it is evident that the fluid passes the orifice under a density less than the density in the discharging vessel.

Again, there is an error in the received theory, in considering the efficient pressure which causes the discharge (represented by P in the above expression), as equal in all cases to the difference of pressure in the two vessels. The true amount which is to be deducted from the pressure in the discharging vessel, in order to find the efficient pressure that produces the discharge, is the elastic force that is due to the density which the fluid has in its passage through the orifice; for it is obvious that that alone reacts against the pressure in the discbarging vessel. From this consideration also we may arrive at the same conclusion as was deduced in the last paragraph, viz: that the fluid must pass the orifice with a diminished density; for otherwise the elastic force of the fluid in the orifice would be a perfect counterpoise to the pressure, and there could be no flow.

From what precedes it will be apparent that in the application of the general expression,

$$VDS \propto \frac{DS\sqrt{P}}{\sqrt{D}}$$

to the case of elastic fluids, the density of the fluid in the orifice, represented by D, is an unknown quantity, having a value somewhere intermediate between cipher and the density of the fluid in the discharging vessel; also that the efficient pressure which produces the discharge, represented by P, is an unknown quantity whose value is dependent on that of D. We will now proceed to elicit a general rule for the determination of the value of D and P in every case that can occur.



In the annexed figure let Δ be the discharging vessel containing fluid whose density is Δ, and let d be the receiving vessel, which for the present we will consider a vacuum. Let the smallest place in the passage leading from one to the other be the orifice, and let its area be S, and let D represent the unknown density with which the fluid passes the orifice. Since the pressure is as the density, the density may be employed to express the pressure. Then it follows from the preceding observations that the efficient pressure which produces the discharge is $\Delta - D$. Since the velocity will be directly as the square root of the pressure and inversely as the square root of the density, we have,

$$V \propto \frac{\sqrt{(\Delta - D)}}{\sqrt{D}}.$$

Multiplying this expression by D and reducing, we have,

$$VD \propto \sqrt{(\Delta D - D^2)}.$$

Now if we conceive several sections to be made across the passage at different points on each side of the orifice, and if the areas of these sections are respectively S', S'', etc., the velocities of the fluid in them V', V'', etc. and the densities D', D'', etc. $V'D'S'$, $V''D''S''$, etc. are the measures of the quantities of fluid that pass through these sections respectively in a given time.

But when the current is established, the same quantity flows through each in a given time. Therefore $V'D'S'=V''D''S''=VDS$. Now VDS being a constant quantity, if each of the factors vary, VD will be a maximum when S is a minimum. But S is a minimum at the orifice; and therefore VD is a maximum at the orifice. But we have before found $VD \propto \sqrt{(\varDelta D-D^2)}$; and therefore when VD is a maximum $\sqrt{(\varDelta D-D^2)}$ must likewise be a maximum. Now, when $\sqrt{(\varDelta D-D^2)}$ is a maximum $D=\frac{\varDelta}{2}$. Hence, when the discharge is into a vacuum, the density of the fluid at the orifice is equal to half the density in the discharging vessel.

For convenience in the illustration, we have made the passage from one vessel to the other in the figure, divergent each way from the orifice; but our reasoning would obviously be equally applicable if the orifice opened directly from one vessel into the other, without the intervention of the divergent tubes.

Let us now inquire, what will be the value of D when the receiving vessel contains fluid of any density less than that in the discharging vessel.

Let the density in the receiving vessel be d. Then d is a limit beyond which the fluid cannot expand, either before or after it passes the orifice; so that D can never be less that d. As in the preceding case $\sqrt{(\varDelta D-D^2)}$ was the maximum for all the values that can be assigned from cipher to $\varDelta$, so in this case, and for the same reasons, $\sqrt{(\varDelta D-D^2)}$ must be the maximum for all the values of D that can be assigned between d and $\varDelta$. But we found in the other case that the maximum occurs when $D=\frac{\varDelta}{2}$. If, then, this value of D is assignable between d and $\varDelta$, the maximum must in this case also occur when $D=\frac{\varDelta}{2}$. But this value of D will always be assignable between d and $\varDelta$, if d be not greater than $\frac{\varDelta}{2}$. Therefore if d be any quantity not greater than $\frac{\varDelta}{2}$, D will be equal to $\frac{\varDelta}{2}$. In other words, if the density in the receiving vessel be not greater than half the density in the discharging vessel, the density in the orifice will be equal to half the density in the discharging vessel.

Again, from the nature of maxima and minima, it is obvious that $\sqrt{(\Delta D - D^2)}$ will be a maximum when, of all the values that are assignable to D, that value is assigned which differs least from $\frac{\Delta}{2}$. Hence, if d exceed $\frac{\Delta}{2}$, so that D must have a value greater than $\frac{\Delta}{2}$, then $\sqrt{(\Delta D - D^2)}$ will be a maximum when D has the smallest value that is assignable to it. Now the smallest value that is assignable to it in this case is $D = d$. Hence, if the density in the receiving vessel exceed half the density in the discharging vessel, the density under which the fluid passes the orifice is equal to the density in the receiving vessel.

Thus we have found for the value of D, $D = \frac{\Delta}{2}$ if d is not greater than $\frac{\Delta}{2}$; if otherwise, $D = d$. And for the value of P (since $P = \Delta - D$) we have $P = \frac{\Delta}{2}$ if d is not greater than $\frac{\Delta}{2}$; if otherwise, $P = \Delta - d$.

In applying the general dynamic law $VDS \propto \frac{DS\sqrt{P}}{\sqrt{D}}$ to the case of elastic fluids, the values of D and P should therefore be assigned in accordance with this rule.

We have already remarked that treatises on the dynamics of fluids, in applying the above general expression to elastic fluids, put D as equal to Δ, and P as equal to $\Delta - d$ in all cases. This, as will appear from the above rule, makes D too large in all cases; and P also too large whenever d is less than half Δ. In the case of a discharge into a vacuum, it makes each of these quantities double what it should be. In constructing a formula to express the *velocity* of the flow into a vacuum, these errors balance each other; so that in that particular case the result is the same as if the values of these quantities were assigned in accordance with our rule; for by our rule $V \propto \frac{\sqrt{\frac{\Delta}{2}}}{\sqrt{\frac{\Delta}{2}}} = 1$, and by the old rule $V \propto \frac{\sqrt{\Delta}}{\sqrt{\Delta}} = 1$.

Again, since $\frac{\sqrt{\frac{\Delta}{2}}}{\sqrt{\frac{\Delta}{2}}}$ is a constant quantity however Δ may vary, it follows from our rule that the *velocity* of the flow into a vacuum is a constant quantity, being the same for every density in the discharging vessel. The same also results from the old theory.

But in constructing a formula to express *the quantity that will flow into a vacuum in a given time,* the results of the two rules will differ widely. For since the rule gives the velocity of the flow correctly, and at the same time puts its density at double what it should be, it follows that the old rule makes the quantity discharged in a given time double what it should be.

Hence it appears that in a steam engine, the valves and pipes which convey the steam from the working cylinder to the condenser, must be of double the size that would be assigned to them by the old rule in order to discharge the contents of the working cylinder in a given time, without increased reaction upon the piston.

It appears from the rule as stated above, for finding the values of D and P, that the quantity *d*, which expresses the density of the fluid in the receiving vessel, will not enter at all into the formula which expresses the velocity of the flow, nor into that which expresses the quantity discharged in a given time, provided it be equal to, or less than, half the density in the discharging vessel. Hence it follows that the fluid in the receiving vessel, if its density does not exceed half the density in the discharging vessel, will have no effect whatever upon the flow. Consequently air or steam will rush into a vacuum no faster than into a vessel containing fluid of half its density. On the contrary, both the velocity of the flow and the quantity discharged in a given time will be the same in both cases; and so, also, if the density in the receiving vessel is any quantity *less* than half the density in the discharging vessel, the flow will be the same in velocity, quantity, and density, as into a vacuum. Accordingly, a vessel containing steam of a density due to a pressure of ten or any other number of atmospheres, will empty itself no faster into a vacuum than into the open air, until in the progress of the discharge, the density is reduced below that due to the pressure of two atmospheres. It will be readily seen that these conclusions have an important bearing upon the construction of the steam engine.

ARTICLE II.

*A Determination of the general law of the Propagation of Pulses in Elastic Media.**

Sir Isaac Newton in his Principia, Book II, prop. 47, 48, 49, has determined the velocity of a pulse, propagated in the atmosphere, whose intensity differs from cipher only by a very small quantity; or rather, as we shall see in the course of this article, whose intensity is cipher. This velocity he shows to be that which a body would acquire by falling over half the atmospherical subtangent (or half the height of a homogeneous atmosphere); and this he assumes would be the velocity of sound, provided the atmosphere were perfectly pure and perfectly elastic. Lagrange and others have since investigated the same case by different processes, but with the same result, and have concurred with Newton in regarding that result as showing the true theoretical velocity of sound. Both Newton and Lagrange in their respective solutions of the case assume, in effect, that the velocity of the pulse is irrespective of its intensity. Newton at one point (prop. 48, case 1), appears to recognize the fact that an increased intensity would make a difference, but thinks that unless the pulse were "exceedingly intense," the error would not be sensible. But Lagrange says (Mecanique Analytique, Part II, sec. 12, art. 14), "the velocity of the pulse is constant and independent of the primitive movement, which is confirmed by experience, as all sounds strong or weak appear to be propagated with the same velocity."

It is proposed in this article to show that the velocities of pulses vary with their intensities, and to determine, in general

*Published A. D. 1848, in the American Journal of Science, second series, vol. v, p. 372.

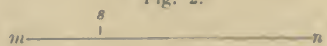
form, the relation which subsists between the velocity and intensity of pulses. This we shall do by solving the general problem of the velocity of pulses by a new process, which comprehends the intensity of the pulse as an essential element. Having done this, we shall see the relation in which the case solved by Newton stands to the general law; and if we mistake not, it will then be apparent that the velocity found in that case is not the velocity of sound, but a *limit* below which its velocity cannot fall.

A pulse, considered as propagated through a line of particles, and considered with reference to its physical condition at any instant of time, consists of a series of contiguous particles in that line, greater or less in number, which are more dense than the particles before and behind them on the line, and which are in motion with some velocity, while the particles before and behind are at rest and in their natural state of density. This series of particles, as it advances, encounters successively the stationary particles, compressing them to the same density, putting them in motion, and thus adds them to the series. In the mean time, an equal number of the posterior particles of the series expand, resuming their natural density, and come to rest. It is obvious that if the propelling force due to the reaction of the particles expanding from the posterior extremity of the series, is equal to the retarding force of the particles encountered by the anterior extremity (as must always be the case if the elasticity is perfect, and if the action is confined to the particles in the line), then the pulse will continue to advance indefinitely, and with a uniform velocity; a velocity however which, as we shall see, is not independent of the degree of condensation to which the particles are brought.

Fig. 1.

Let C be a point where the density is a maximum in a pulse which is moving toward B. It is not material to our present purpose to inquire whether the place of maximum density is a mere point, or whether it extends over some finite space on the line AB. In either case, somewhere in advance of C we shall find particles in every stage of density from the natural to the maximum state; and these will be arranged in the order of their density, the more dense being toward C. Each of these particles will be accelerated so long as the density of the particle behind it is greater than that of the particle before it, and no longer. Consequently, each will have its maximum velocity when it reaches its maximum density. Therefore, if C is a point where the density is a maximum, it is likewise a point where the velocity, which the pulse gives successively to all the particles, is a maximum.

Fig. 2.



Let *mn* be the space which a particle in its natural state occupies on the line AB, fig. 1 and let *sn* be that which it occupies in its most condensed state. Let D be a point in the line AB, in advance of C, where the particles are at rest, not having yet felt the influence of the approaching pulse; and let the two imaginary points C and D be conceived to move with the same velocity as the pulse. Then each travels *over* the same space and *through* the same number of particles in a given time. Consequently while C moves over a space equal to that occupied by a particle in its natural state, it only moves *through* one particle in its most condensed state; that is to say, while C moves over the space *mn*, it moves, relatively to the particle through which it passes, only over *sn*. Consequently the particle itself moves in the same time over a space equal to *ms*. Hence when *mn* represents the velocity of the pulse, *ms* represents the final velocity which the pulse gives to every particle through which it passes.

Let H be the atmospherical subtangent; or the length of the column of particles of the natural density whose weight is equal to the elastic force of a particle in its natural state, and let $H+h$ be the length of a similar column whose weight is equal to the elastic force of a particle at the maximum density. Since the space occupied by a particle is inversely as the compressing force, we have,

$$mn : sn :: H+h : H \quad \text{or} \quad ms+sn : sn :: H+h : H ;$$

consequently $$ms : sn :: h : H. \quad \text{Whence} \quad H=\frac{sn \times h}{ms}$$

The force which accelerates all the particles in advance of C, which have felt the influence of the pulse but have not yet reached their maximum velocity, is the difference of the elastic forces which correspond to the natural and the maximum densities. This is a constant force, and must evidently be that force which is competent to give the velocity *ms* to all the particles in

3

any space in the time in which the pulse runs over that space. Let us suppose the pulse runs over the space h. Then the pulse runs over h in the time in which the difference of those elastic forces will give to all the particles in h the velocity ms. But the difference of the elastic forces is equal to the weight of all the particles in h. Therefore the pulse runs over h in the time in which those particles would in falling by their own weight acquire the velocity ms.

The time in which a falling body acquires the velocity ms is to the time in which it would acquire the velocity of the pulse or mn, as ms to mn, and the spaces over which the pulse would run in these times are as the times and therefore as ms to mn. Therefore putting S for the space which the pulse would run over while a falling body would acquire the velocity of the pulse, we have $ms : mn :: h : S$. Whence

$$S = \frac{mn \times h}{ms} = \frac{\overline{ms + sn} \times h}{ms} = \frac{sn \times h}{ms} + h \cdot$$

But we have before found $H = \frac{sn \times h}{ms}$. Substituting H for its value in the preceding equation, we have $S = H + h$. If then we put $H + h$ for the velocity of the pulse, the space through which a body must fall to acquire that velocity will be $\frac{H+h}{2}$.

In this expression h is to be regarded as the *intensity* of the pulse: it being the length of that column of particles which must be superadded to the height of a homogeneous atmosphere, in order to produce in the air that degree of increased condensation which the pulse effects in the particles through which it passes.

If in the expression last found we make $h = 0$, the expression becomes $\frac{H}{2}$. This is the result arrived at by Newton, and which, as we have already remarked, was regarded by him and is now generally received as the theoretical formula for the space through which a body must fall to acquire the velocity of sound. But it is evident from our demonstration that the velocity due to that space, instead of being the velocity of any assignable pulse is simply a limit below which no pulse can be propagated in an elastic fluid whose subtangent is H.*

* Newton's demonstration of this problem has been regarded by several distinguished mathematicians as obscure and inconclusive. It commences with the hypothesis that a particle put in motion by a pulse is accelerated and retarded

A pulse which produces the sensation of sound must produce real motion in the particles through which it passes. In such a pulse h must have some finite magnitude. Nor can that magnitude be by any means the smallest that is competent to produce motion in the air; for if such were the fact, then the slightest impulse given to the air by a vibratory movement, even waving the hand in it, should produce the sensation of sound. The intensity of a pulse which is competent to produce that sensation, will of course vary with the sensibility of the ear which is to receive it; and consequently the nature of the case does not allow us to assign any definite magnitude to the minimum intensity of sonorous pulses; but we know by experience that the velocity of the particles in which a pulse originates must be great in order to produce the sensation of sound in the most delicate ear. We also know that in the case of the heavier sounds, as the report of cannon, the condensation of the particles in which the pulse originates is very intense.

This view of the subject may throw some light upon the discrepancy between the theoretical velocity of sound, as determined by Newton and others, and its real velocity as found by experiment. The velocity according to that theory should be about 944 feet in a second; varying slightly from this according to the state of the barometer, thermometer, and hygrometer.

The velocities found by experiment, by	Roberts,	1300
	Boyle,	1200
	Mersenne,	1474
	Flamsteed and Halley,	1142
	Florentine Academy,	1148
	French Academy,	1172

according to the law of the oscillating pendulum. Gabriel Cramer (see Glasgow edition of Newton's Principia, Book II, prop. 48, notes), objects to the result arrived at by Newton, that it flows from his hypothesis and not from the nature of things. To prove this he deduces the same result upon the hypothesis that the particle is accelerated and retarded by a constant force. The fact that Cramer's hypothesis answered just as well for the solution of the problem as Newton's, seems not a little to have puzzled the editor of the edition of the Principia referred to, who devotes several pages of notes to the vindication of Newton's result from its supposed bearing. But the enigma is solved when we consider that both Newton and Cramer regard the space through which the particle vibrates as an infinitesimal quantity. In such case, evidently it can make no difference what is assumed as the law of acceleration; this being a point at which all laws coalesce.

Newton adopts the smallest of these experimental velocities, viz: 1142 feet per second, as the true practical velocity of sound; and to this he reconciles his theory; in part, by a hypothesis that the air consists, to a certain extent, of solid particles, through which the pulse is transmitted instantaneously; and in part by another hypothesis, that the pulse does not give motion to the foreign matter which the air contains, and so is transmitted so much the faster through the true air as there is less of it in a given space. This explanation of the matter has not been satisfactory to those who have followed Newton in investigating this subject. They have justly thought that such causes might retard, but could not accelerate the pulse. Various other hypotheses have been successively proposed and rejected, and a vast amount of labor has been expended in the effort to reconcile theory and practice in this case. Of these hypotheses, we will mention only that which Laplace is said to have regarded as the true one, viz: the increased elasticity in the air produced by the heat evolved by condensation. A little reflection will serve to show that this cause also may retard, but cannot accelerate the pulse. In order that the force of the pulse may be maintained without loss, the propelling force derived from the reëxpansion of the particles must be not less than their retarding force in being compressed; and in order to this, whatever heat is evolved during their compression, must be reabsorbed in their reëxpansion. If, then, any *time* is required either for the evolution of the heat, or for its absorption, so that the specific heat of the particle does not instantaneously conform to the change of density; or if any portion of the heat evolved is radiated and lost, so as not to be present to be reabsorbed, then the evolution of heat must retard the pulse. Otherwise it cannot affect the velocity of its propagation.

In the view of the subject we have taken, it will cease to be a matter of surprise that the velocity of sound should be found to be greater than that assigned by Newton's theory; as, also, that the experimental velocities should be found to differ greatly among themselves, however carefully the experiments may have been tried.

When the velocity of a pulse is given, we may find its intensity by the formula $h = \frac{V^2}{32} - H$. We may find the maximum density, the natural density being 1, by the formula, max. dens. $= \frac{V^2}{32H}$.

We may find the space occupied by a particle at the maximum density, its natural extent being 1, by the formula, extent at max. dens. $= \frac{32 H}{V^2}$.

When the velocity is 1142 feet per second, if we put the subtangent $H = 27818$ feet, we have,

Intensity of the pulse, $h = 12937$ feet.
Maximum density, 1·465.
Extent of particle, 0·682.

If we take for the given velocity, that which a body would acquire by falling through the subtangent, or that with which air would rush into a vacuum, we shall find $h = H$, and the particles will be compressed into half their natural size, and the density will be double the natural density.

A condensing force equivalent to the pressure of a column 12937 feet high, and which compresses the particles into about $\frac{68}{100}$ of their natural size, is a pulse "exceedingly intense" as compared with that which Newton supposes; but if our solution of the problem is correct, it is physically impossible that a pulse of less intensity should propagate itself in our atmosphere with a velocity of 1142 feet in a second.

ARTICLE III.

---

*On the Mode of Expansion of Elastic Fluids as controlled by Dynamic Laws.**

That under the controlling influence of the known laws of motion, elastic fluids must expand according to some definite and invariable law, is an obvious truth and one which has often been recognized by mathematicians. But the determination of that law is a problem which hitherto, it is believed, has not been solved. There are many interesting points in mechanics and physics, in relation to which the present state of knowledge is imperfect, which depend for their correct and complete development, in part at least, on a solution of this problem. It is therefore a point of some interest to science. It is our purpose in this article to solve this problem; and we shall do so by employing a method similar in part to that employed in solving the problem of the propagation of pulses in elastic media, in the American Journal of Science, second series, vol. v, page 372.

Before entering upon the investigation we will here state one curious and remarkable fact which the investigation discloses. We revert to it here because a fact so much at variance with preconceived notions may be interesting to those readers who will not care to follow out the mathematical details of this article.

When a fluid passes by free expansion from one state of density to another, we should naturally suppose that it must pass through all the intermediate states of density that can be assigned between the two. Such appears to have been the notion of every writer who has made reference to this point; and at first view it would seem absurd to suppose that the fact could be otherwise. But

*Published A. D. 1850, in the American Journal of Science, second series, vol. ix, p. 334.

such is not the way in which elastic fluids expand. On the contrary the parts of the fluid successively and *instantaneously* change their density, to the extent of one-half (when free to expand to that extent) *without passing into the intermediate states.* As vapor is thrown off from the surface of water in a tenuous state *ab initio*, and without having first passed into those states of density which are intermediate between the density of the water and that of the vapor, so a column of rarefied fluid is thrown of from the front of a denser column; each infinitesimal element of the highest order of the denser column, being successively and instantaneously transformed to the more rare state. And as the change of density is instantaneous, so likewise the entire velocity due to that change is imparted instantaneously to each element successively.

But to proceed with the investigation; suppose a straight tube of uniform calibre extending indefinitely in both directions from a given point. Suppose the tube on one side of this point to be filled with a column of fluid of the density D, indefinitely expansible, and always maintaining the same ratio between its density and elastic force when it expands; and suppose the other portion of the tube a perfect vacuum. It is required to determine the law according to which the fluid expands into the vacuum; so that we may be able to assign the precise state of the fluid, in respect to density and velocity, at each and every point of the tube after the lapse of any given time from the commencement of the expansion.

Since the elastic force is always as the density, D may represent both the density and the elastic force. The force D acts during the first instant in every part of the column, and in *every direction;* and therefore during that instant every part of the column is kept in equilibrio except the first element. Consequently in the first instant expansion takes place in the first element only, and as the whole force D acts during that instant, the parts of this element must receive such velocities that the sum of their momenta shall be equal to that due to the action of the constant force D during that time. It is obvious that the termination of the first instant coincides with the commencement of motion in the second element; also that motion will not commence in the second element until the density in front of it has been to some extent reduced. Let the ratio in which it is reduced before motion begins in the second element be represented by $\frac{1}{x}$.

Then the density of the posterior part of the expanded element at the end of the first instant is $\frac{D}{x}\cdot$ Now for reasons which will soon be apparent, all the other parts of the expanded element, whatever may be their present state of density, may be considered as having passed first into the density $\frac{D}{x}\cdot$ But at the same time that the grade $\frac{D}{x}$ began to form in front of the column, that grade itself must have begun to expand *again* in the same ratio, forming another grade $\frac{D}{x^2}\cdot$ And at the same time that the grade $\frac{D}{x^2}$ began to form, that likewise must have begun to expand in the same ratio forming a grade $\frac{D}{x^3}$, and so on *ad infinitum*. The grades therefore will correspond to the terms of an infinite series, in decreasing geometrical progression. All of them originate *simultaneously* in the first element; and yet every grade respectively may be considered as having passed into and out of all the grades which precede it; inasmuch as each in its origin is a *constituent part* of that which precedes it. The fluid which passes into any one of these grades in the first instant does not all of it pass into the next in the same time; for equal quantities by *measure* expand in equal ratios in equal times; and since a given quantity by measure in any one grade becomes a larger quantity by measure when expanded into the next grade, a portion will have been left at the end of the first instant in each grade which has not expanded into the next. Hence at the end of the first instant the first element of the column will have been distributed into portions or grades, having their respective densities corresponding to the terms of the infinite series

$$\frac{D}{x}, \frac{D}{x^2}, \frac{D}{x^3}, \frac{D}{x^4}, \text{ etc.} \qquad \text{(A)}$$

If we extend this series backward one term, we obtain the series

$$D, \frac{D}{x}, \frac{D}{x^2}, \frac{D}{x^3}, \frac{D}{x^4}, \text{ etc.} \qquad \text{(B)}$$

Since equal quantities by measure pass out of each of these states in a given time, if s be the space occupied by the original element, and if we multiply each of the terms of the series (B) by s, then the terms of the resulting series

$$Ds,\ \frac{Ds}{x},\ \frac{Ds}{x^2},\ \frac{Ds}{x^3},\ \frac{Ds}{x^4},\ \text{etc.} \qquad (C)$$

will severally express the quantities of fluid that expand from each grade respectively into the next. Now since fluids expanding in equal ratios acquire equal velocities, equal velocities are acquired in each of these expansions. If then we find that velocity and by it multiply the sum of the series (C), the product will be the sum of the momenta generated in, or imparted to, the parts of the first element in the time in which the point of expansion recedes through s.

If the quantity Ds be expanded from the density D to the density $\frac{D}{x}$, the space it will occupy will be increased in the inverse ratio of these densities; and, therefore, $\frac{D}{x} : D :: s : sx$. Hence s and sx are respectively the spaces occupied by the element before and after the first expansion. Now the velocity which the mass Ds receives in this expansion, is obviously that which would carry it over the difference between these spaces in the time in which the expansion takes place; that is, the velocity imparted in the first expansion is $sx - s = s.\overline{x-1}$; and the same velocity is imparted in every other expansion. If, then, we multiply the sum of the series (C) by $s.\overline{x-1}$, the product will be equal to the sum of all the momenta generated in the parts of the element. This product is Ds^2x. Therefore Ds^2x is the entire amount of momentum which the force D is competent to generate in the time in which the point of expansion recedes through s.

We will now proceed to find another expression for the momentum which the force D is competent to generate in the same time, in order that by comparing it with that just found, we may ascertain the value of x.

Let H be the height of a column of fluid of the density D, whose weight is equal to the elastic force D; and let $H - h$ be the height of another column of the same density whose weight is equal to the elastic force $\frac{D}{x}$. Then $\frac{D}{x} : D :: H : H - h$. Let mn be the space occupied by the first element at the density D,

and ms, that which it occupies when expanded to the density $\frac{D}{x}$. Since the spaces occupied by the element in these states are inversely as the densities, $mn : ms :: \frac{D}{x} : D :: H-h : H$, and therefore, $ms-sn : ms :: H-h : H$; whence we obtain $H = \frac{h \times ms}{sn}$. In the time in which the point of expansion recedes through mn, the element Ds receives a velocity which will carry it over sn in the same time. If, then, mn represent the velocity of the point of expansion, sn will represent the velocity imparted to the fluid by the first expansion. Consequently, the retrogressive velocity of the point of expansion must be such that in the time in which it passes over any space, the force $D-\frac{D}{x}$ may give to all the fluid in that space the velocity sn. Hence the point of expansion will run over h in the time in which the force $D-\frac{D}{x}$ will give to all the fluid in h the velocity sn. But the force $D-\frac{D}{x}$ is equal to the *weight* of all the fluid in h. Therefore the point of expansion runs over h in the time in which the mass h would in falling by its own gravity acquire the velocity sn. The time in which a falling body acquires the velocity sn is to that in which it would acquire the velocity of the point of expansion, or mn as sn to mn; and the spaces over which the point of expansion would run in these times are in the same ratio. Therefore, putting S for the space which the point of expansion would run over while a falling body would acquire the velocity of the point of expansion, we have $sn : mn :: h : S$, or, $sn : ms - sn :: h : S$; whence we obtain $S = \frac{h \times ms}{sn} - h$. But we have before found $H = \frac{h \times ms}{sn}$. Therefore $S = H - h$; that is, the point of expansion will run over $H-h$ in the time in which a falling body will acquire the same velocity. Consequently the velocity of the point of expansion is that which a body will acquire by falling through $\frac{H-h}{2}$.

If the force D act on the mass H during the time that mass would fall through H, it would give that mass a velocity which would carry it over 2H in the same time, because the force D is equal to the weight of the mass. The mean velocity of a body falling through H is that which will be acquired by falling through

$\frac{H}{4}$. If, then, the point of expansion moved with the velocity acquired by a body in falling through $\frac{H}{4}$, in the time of passing over H, the force D would be competent to give to all the fluid in H a velocity which would carry it over 2H in the same time; and, consequently, in the time of passing over s it would give to the mass Ds a velocity which would in the same time carry it over $2s$. But the point of expansion, as before shown, moves with the velocity acquired by falling through $\frac{H-h}{2}$. Now the velocity due to $\frac{H}{4}$ is to that due to $\frac{H-h}{2}$ as $\sqrt{\frac{H}{4}}$ to $\sqrt{\frac{H-h}{2}}$; and the times in which the point of expansion would move over s with these velocities, are inversely as these velocities, or as $\sqrt{\frac{1}{\frac{H}{4}}}$ to $\sqrt{\frac{1}{\frac{H-h}{2}}}$. But $D:\frac{D}{x}::H:H-h$, and therefore these times are as $\sqrt{\frac{1}{\frac{D}{4}}}$ to $\sqrt{\frac{1}{\frac{D}{2x}}}$, or as 2 to $\sqrt{2x}$. The velocity which the force D can impart in these times is as the times respectively. And since it has been shown that in the former of these times the velocity $2s$ will be imparted by the force D, we have $2:\sqrt{2x}::2s:\frac{2s\sqrt{2x}}{2}=s\sqrt{2x}$. That is, the *velocity* which the force D is competent to impart to the mass Ds in the time in which the point of expansion recedes through s, is $s\sqrt{2x}$. Consequently the *momentum* which the force D can impart in the same time is $Ds^2\sqrt{2x}$. But we have before found this momentum to be Ds^2x. Therefore $Ds^2x=Ds^2\sqrt{2x}$, whence $x=\sqrt{2x}$ and $x=2$.

Having thus found the absolute value of x, if we substitute this value for x in the series (A) we shall have, for the densities of the several parts or grades into which the first element will have been distributed at the end of the first instant, the respective terms of the following series, viz:

$$\frac{D}{2}, \frac{D}{4}, \frac{D}{8}, \frac{D}{16}, \frac{D}{64}, \text{ etc.}$$

We found the velocity of the point of expansion to be that which a body will acquire by falling through $\frac{H-h}{2}$; the value of h being dependent on the value of x. But when $x=2$, $\frac{H-h}{2}=\frac{H}{4}$. Therefore the absolute velocity of the point of expansion is that which a body will acquire by falling through $\frac{H}{4}$, or one-fourth of the subtangent of the fluid.

Since the extent of the element is doubled by the first expansion, the velocity of the first grade will be equal to the velocity of the point of expansion, or that due to one-fourth of the subtangent; and an equal additional velocity is imparted in each succeeding expansion. If, then, v represent the velocity due to one-fourth the subtangent of the fluid, the absolute velocities of the several grades respectively will be expressed by the respective terms of the series v, $2v$, $3v$, $4v$, $5v$, etc.

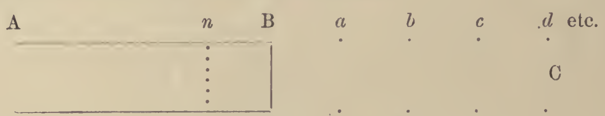
Since one element $=s$ by measure passes from each grade into the next, and becomes $=2s$ in the next, the length of each grade at the end of the first instant $=2s-s=s$. That is, the length of each grade is equal to that of the original element; and the *place* of the first grade is that which was occupied by the original element, the other grades succeeding it in continuous order.

Having now ascertained the state of things at the end of the first instant, let us inquire what takes place in the second instant.

It is obvious that during the second instant the front of the second element of the column, and also the front of each grade respectively is a point of expansion from which one element $=s$ by measure passes into the next grade. Thus in the second instant each grade receives an addition of $2s$ to its rear and loses $1s$ from its front. The same takes place in every succeeding instant. Since the increment of the length of the grades for each instant is s, the velocity of the increase is v. The length of the grades is therefore always equal to the space through which the point of expansion has receded in the column. Thus while the length of the grades increases with the uniform velocity v, their number, velocity and density remain unchanged. Consequently no other gradations of density can exist in front of a column expanding into a vacuum, but those which correspond to the terms of the infinite geometrical series $\frac{D}{2}$, $\frac{D}{4}$, $\frac{D}{8}$, $\frac{D}{16}$, etc.; and no other gradations of velocity but those which correspond to the terms of the infinite arithmetical series v, $2v$, $3v$, $4v$, etc.

The point of expansion in the column recedes with the velocity v; and since the length of the first grade is always equal to the space through which that point of expansion has moved, it follows that the point of expansion from the first to the second grade is stationary. And since the second grade increases in length with the velocity v, the third point of expansion moves forward with the velocity v; and since all the other grades increase in length with the same velocity v, the velocities of the several points of expansion will be expressed by the following series—v, 0, v, $2v$, $3v$, $4v$, etc.

In order to give a synopsis of the results to which we have come, let AB be a column of fluid of the density D, expanding into a vacuum toward C. Let the velocity due to a height equal to one-fourth of the subtangent of the fluid be v. Suppose expansion to have commenced at B, and the point of expansion to have receded to any distance n. Set off from B an infinite number of spaces Ba, ab, bc, cd, etc., each equal to Bn. Then the points n, B, a, b, c, d, etc., are the places of the points of expansion, and the boundaries of the several grades, or parts having different degrees of density and velocity, into which the original mass Bn has been distributed.



Between these points respectively the densities are

$$\frac{D}{2} \cdot \frac{D}{4} \cdot \frac{D}{8} \cdot \frac{D}{16} \cdot \frac{D}{64} \cdot \text{ etc.}$$

The velocities are

$$v \,.\, 2v \,.\, 3v \,.\, 4v \,.\, 5v \,.\, \text{etc.}$$

These points move toward C with the velocities

$$-v,\ 0,\ v,\ 2v,\ 3v,\ 4v,\ \text{etc.},$$

and relatively to each other, and to the fluid, with the velocities

$$v,\ v,\ v,\ v,\ v,\ v.$$

As corollaries from the preceding investigations we may state the following propositions:

1. No other gradations of density can exist in front of a column of fluid which is expanding toward a vacuum except those which are found by successive divisions of the original density by 2.

2. The change of density in the fluid in passing from one of these grades to the next is not *gradual* but *instantaneous;* so that the grades are constantly separated from each other by a mere imaginary plane.

3. No other velocities can exist among the parts of a fluid which is expanding toward a vacuum but such as are multiples of the velocities which a body will acquire by falling through one-fourth of the subtangent of the fluid.

4. The velocity imparted to the particles of an expanding fluid is not the result of a continual and gradual acceleration, but of successive instantaneous increments equal to that which a body will acquire by falling through one-fourth of the subtangent of the fluid.

It now remains to consider the mode of expansion when the fluid is not free to expand indefinitely, but has its expansion arrested at some given density d.

It is obvious that if d correspond in value to any of the terms of the series, the manner of expansion up to that point will be the same as if the expansion were continued indefinitely. There will therefore be in the expanding fluid, in such case, so many grades corresponding to the terms of the series; as there are of complete terms intervening between D and d. But let us inquire what takes place when d does not correspond to any terms of the series. First, suppose d to be greater than the first term. Then from what has been before shown, the velocity of the point of expansion is that which a body will acquire by falling through $\frac{H-h}{2}$ when $H-h$ is the height of a column whose weight is equal to the elastic force of the expanded fluid; also that the velocity of the point of expansion is that due to the height $\frac{H}{4}$ when the expansion is from D to $\frac{D}{2}$. These velocities are as $\sqrt{\frac{H}{4}}$ to $\sqrt{\frac{H-h}{2}}$ and since $H : H-h :: D : d$, those velocities are as $\sqrt{\frac{D}{4}}$ to $\sqrt{\frac{d}{2}}$. The velocity due to $\frac{D}{4}$ is v. Hence we have $\sqrt{\frac{D}{4}} : \sqrt{\frac{d}{2}} :: v : v\sqrt{\frac{2d}{D}} =$ velocity of the point of expansion in this case.

Let us next find the velocity of the fluid. The times of running over s by the point of expansion, with the velocities $\sqrt{\frac{D}{4}}$ and $\sqrt{\frac{d}{2}}$ are inversely as these velocities; and the velocities imparted to the mass Ds in these times are as the products of the times by the respective forces. When the velocity of the point of expansion was $\sqrt{\frac{D}{4}}$ the force was $\frac{D}{2}$ and the velocity of the fluid was v. The force in the present case is $D-d$. Hence we have $\frac{\frac{D}{2}}{\sqrt{\frac{D}{4}}} : \frac{D-d}{\sqrt{\frac{d}{2}}} :: v : v\sqrt{2}\,.\,\frac{D-d}{\sqrt{Dd}} =$ velocity of the fluid in this case.

Secondly, suppose the value of d to fall between any two consecutive terms of the series. It is obvious that we have now only to substitute in the expression last found that term of the series which is next greater than d for D, and it will then express the acceleration due to expansion from the last complete term into the fractional grade.

To find the retrogressive velocity of the point of expansion, relatively to the fluid, in the grade which precedes the fractional grade, we must make the like substitution of the last complete term for D in the quantity $v\sqrt{\frac{2d}{D}}$ found above. The retrogressive velocity of the point of expansion in the grade which precedes the fractional grade is greater than in the other grades, and of course that grade will be shorter than the others in the same ratio. This is the only modification which a fractional grade produces in those that precede it. In all other respects the mode of expansion, up to the fractional grade, corresponds to the view presented in the foregoing synopsis.

We are now prepared to construct a formula for the final velocity of a fluid which expands from any density D to any other density d.

Let V be the final velocity; v the velocity due to a height equal to one-fourth the subtangent of the fluid; n the number of complete terms of the series $\frac{D}{2}, \frac{D}{4}, \frac{D}{8}, \frac{D}{16}$, etc., which intervene between D and d. Then vn is obviously the velocity of the grade which precedes the fractional grade, if there be a fractional grade. When the first grade is fractional we found its velocity to be $v\sqrt{2}\,.\,\frac{D-d}{\sqrt{Dd}}$; and we also found that to suit this expression to the case of a fractional grade occurring elsewhere in the range of the series, we are to substitute for D that term of the series which is next greater than d. Now the value of that term will be $\frac{D}{2^n}$. Making the substitution accordingly, the expression for the additional velocity due to expansion into the fractional grade becomes, after reducing $v\sqrt{2}\,.\,\frac{D-2^n d}{\sqrt{2^n Dd}}$. By adding this quantity to vn we obtain the final velocity of the fluid, resulting from its expansion from any density D to any other density d. Hence the formula is

$$V = v\,.\,\overline{n + \sqrt{2}\,.\,\frac{D-2^n d}{\sqrt{2^n Dd}}}.$$

When there is no complete term of the series between D and d, $n = 0$ and the above formula becomes

$$V = v\sqrt{2}\,.\,\frac{D-d}{\sqrt{Dd}}.$$

When there is no fractional grade, that is, when d is equal to some term of the series, that part of the formula beyond n equals 0, and then the above formula becomes $V = vn$.

From the general principles here developed it is obvious that, as in expansion, so likewise in *condensation*, the transition of an elastic fluid from one density to another is not by gradations which may be represented by a curve, but abrupt, instantaneous, *per saltum vel saltus*. Pulses, therefore, which are propagated in elastic fluids partake of the same character; that is, the condensation and subsequent reëxpansion of the successive elements through which the wave moves is instantaneous. This fact was not known when the article on the propagation of pulses, referred to at the commencement of this article, was written. It however does not affect the validity of the reasoning in that article.

ARTICLE IV.

Experimental Demonstration of the Law of the Flow of Elastic Fluids which was deduced theoretically in Article I.*

In volume V, second series, of the American Journal of Science, page 78, I proposed a new theory of the flow of elastic fluids through orifices, differing essentially from that heretofore received. The chief object of the present article is to give an account of an experiment instituted for the purpose of testing the truth of that theory.

The fundamental points of difference between the old theory and the new, are as follows:

1. The old theory regards the constant force which expels the fluid as being, in all cases, equal to the difference between the elastic forces of the fluids in the two vessels.

The new theory regards it as equal to that difference only when the less exceeds half the greater; and in all other cases as equal to half the greater.

2. The old theory considers the fluid as passing the orifice with a density equal to that in the *discharging* vessel.

The new theory considers it as passing the orifice with a density equal to that in the *receiving* vessel, whenever this last is equal to or greater than half the density in the discharging vessel; and in all other cases with half the density in the discharging vessel.

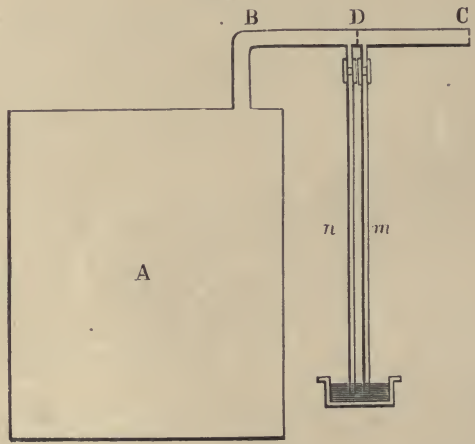
The formula for the quantity discharged in a given time, predicated upon the new theory, gives, in all cases, less than

* Published A. D. 1851, in the American Journal of Science, second series, vol. xii, p. 186.

that predicated upon the old theory. In the case of a flow into a vacuum, the difference amounts to one-half.

The scheme devised to test the relative merits of the two theories, was founded upon the following considerations, viz: When air rushes from the atmosphere into a receiver wholly or partially exhausted, passing on its way through a small intermediate vessel or chamber, entering that chamber and passing out of it through equal orifices, it will take in that chamber a density somewhere intermediate between that of the atmosphere and that in the receiver. For each relation that may at any moment subsist between the density of the atmosphere and that in the receiver, the density in the chamber will have a certain definite and determinate value, such that the chamber may receive through one orifice and discharge through the other simultaneously the same quantity of air. Now since in order to this equal simultaneous flow the two theories respectively demand quite different densities in the chamber, the object of my experiment was to ascertain the actual densities in such a chamber under various relations of the density in the receiver to the density of the atmosphere, in order to compare the densities thus ascertained experimentally with those demanded by each theory respectively in like circumstances.

Fig. 1.



To try the experiment, I constructed the apparatus shown in figure 1. A is a vessel or receiver of the capacity of about fifty gallons, so arranged that it may be exhausted by the air-pump or otherwise. B is an elbow formed of lead pipe of one inch calibre, one branch of which opens into the receiver, and the end of the other branch at C is covered by a brass plate or disc about $\frac{1}{16}$th of an inch in thickness, through which is an orifice of about $\frac{1}{16}$th of an inch in diameter. Another similar plate with an orifice of the same size intersects the pipe at D, thus forming a chamber between the two plates. Two short tubes are inserted into the lower side of the pipe; one on each side of the plate D. With these short tubes two glass tubes m and n, each thirty-three inches in length, are connected by means of pieces of India rubber hose. These glass tubes are open at both ends and terminate at the bottom in a vase of mercury. A rod (not shown in the sketch) graduated to inches and tenths is placed beside the glass tubes, sustained upon a float resting upon the surface of the mercury, so adjusted that zero of the graduation may coincide with the surface of the mercury.

If the orifice at C be closed by a stopper and the receiver exhausted, the mercury will rise in the tubes; and if the density of the atmosphere at the time of the experiment be expressed in inches of mercury, the height of the mercury in the tubes as read upon the graduated rod will be equal to the difference between the density of the atmosphere and that in the receiver. If we now remove the stopper from the orifice at C, the column of mercury in the tube m will instantly subside to a point which indicates the difference between the density of the atmosphere and the density in the chamber when an equal quantity of air flows through the two orifices; while at the same time the column of mercury in the tube n will only have begun to subside very slowly as the density in the receiver increases. Having noted the height of the barometer at the time of the experiment, if we note the simultaneous heights of these two columns of mercury, and deduct them respectively from the height of the barometer, we shall have the density in the chamber necessary to an equal flow through the two orifices under the relation which subsists at the moment of notation between the density in the receiver and the density of the atmosphere. And if we note the simultaneous heights of these columns at various times during the filling of the receiver, so many densities in the chamber shall we find corresponding to the different relations of the other two densities.

At the time of the experiment the height of the barometer, or density of the atmosphere was thirty inches. In consequence of leaks in the receiver, I was unable to exhaust it so as to raise the column in the tube n higher than twenty-six inches. I noted the simultaneous altitudes of the two columns at the moment when the column n coincided with each successive inch-mark upon the graduated rod, and thence ascertained the densities in the chamber under twenty-six different relations between the density in the receiver and that of the atmosphere. These results I have placed in the table beyond, in which the first column shows the densities in the receiver at the times of notation, and the second the densities in the chamber corresponding thereto.

In order to ascertain what these densities should have been according to the old theory, I constructed a formula as follows: Let Δ be the height of the barometer at the time of the experiment, D the density in the receiver, d the density in the chamber, V the velocity through the first orifice, v the velocity through the second orifice. Then according to the old theory the force which drives the air through the first orifice is $\Delta - d$ and that which drives it through the second orifice is $d - \mathrm{D}$. But since an equal quantity flows through both, these forces are as the velocities, that is $\Delta - d : d - \mathrm{D} :: \mathrm{V} : v$.

Again, according to the old theory the density with which the air passes the first orifice is Δ, and that with which it passes the second orifice is d. But since the orifices are equal and the quantities which pass through them are also equal, the products of the velocities by the densities are equal, that is $\Delta \mathrm{V} = dv$ and $\mathrm{V} = \frac{dv}{\Delta}$. Substituting this value of V in the preceding couplet and then finding the value of d, we have the following formula for determining the densities in the chamber according to the old theory, viz :

$$d = \sqrt{\Delta^2 + \frac{\overline{\Delta - \mathrm{D}}|^2}{4}} - \frac{\Delta - \mathrm{D}}{2}.$$

The several densities in the chamber computed by this formula are placed in the fourth column of the table.

In order to ascertain what the densities in the chamber should have been according to the new theory, I constructed a formula as follows, preserving the same notation as above.

By the new theory the force which drives the air through the first orifice is $\Delta - d$ whenever d is not less than $\frac{\Delta}{2}$. But d is never less than $\frac{\Delta}{2}$ when an equal quantity flows through both orifices, for if it were so the chamber would, according to our theory, be receiving as much as could flow into a vacuum under the pressure Δ, and must therefore discharge into the receiver as much as would flow into a vacuum under a pressure Δ; in order to which the density in the chamber must be equal to Δ, and therefore greater than $\frac{\Delta}{2}$. Consequently, the force which drives the air through the *first* orifice is in this arrangement always $\Delta - d$. Again, the force expended in driving the air through the second orifice by the new theory is $\frac{d}{2}$ whenever D is not greater than $\frac{d}{2}$. Let us first construct a formula for the cases in which D is not greater than $\frac{d}{2}$. In these cases the densities under which the air passes the orifices are respectively $\Delta - d$ and $\frac{d}{2}$. Since the forces are as the velocities,

$$\Delta - d : \frac{d}{2} : : \mathrm{V} : v \, ;$$

and since the quantities are equal, $d\mathrm{V} = \frac{dv}{2}$, and $\mathrm{V} = \frac{v}{2}$. Substituting this value of V in the couplet, we have $d = \frac{4}{5}\Delta$; a constant quantity. Hence while the density in the receiver varies from 0 to $\frac{2}{5}\Delta$, the density in the chamber is a constant quantity and equal to $\frac{4}{5}\Delta$. Let us now construct a formula for finding the value of d when D is greater than $\frac{d}{2}$. In these cases the forces are $\Delta - d$ and $d - \mathrm{D}$ and we have for the couplet $\Delta - d : d - \mathrm{D} : : \mathrm{V} : v$. The densities in the orifice are d and D and we have $d\mathrm{V} = \mathrm{D}v$ and $\mathrm{V} = \frac{\mathrm{D}v}{d}$. Substituting this last quantity in the couplet we find

$$d = \sqrt{\mathrm{D}^2 + \frac{\overline{\Delta - \mathrm{D}}|^2}{4}} + \frac{\Delta - \mathrm{D}}{2}$$

as the formula for the value of d by the new theory when D exceeds $\frac{2}{5}\Delta$. The densities in the chamber computed by these formulæ are placed in the third column of the table.

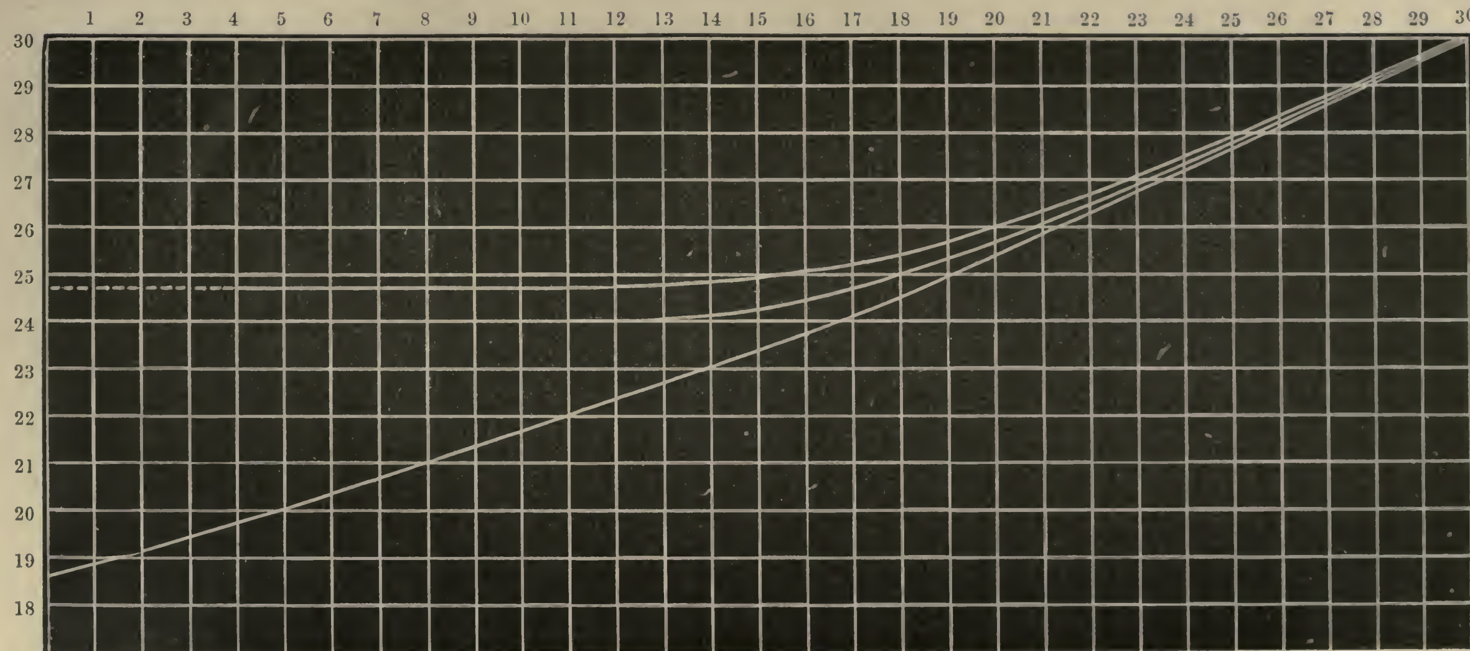
DENSITY OF THE ATMOSPHERE DURING EXPERIMENT 30.

Density in the Receiver.	Density in the chamber as found by experiment.	Density in the chamber due to the new theory.	Density in the chamber due to the old theory.	Deviation of the experiment from the new theory.
0	----	24	18.541	----
1	----	24	18.820	----
2	----	24	19.106	----
3	----	24	19.397	----
4	24.58	24	19.695	.58
5	24.58	24	20.	.58
6	24.58	24	20.311	.58
7	24.58	24	20.628	.58
8	24.58	24	20.953	.58
9	24.58	24	21.284	.58
10	24.58	24	21.622	.58
11	24.60	24	21.968	.60
12	24.64	24	22.320	.64
13	24.70	24.032	22.713	.668
14	24.77	24.124	23.048	.646
15	24.89	24.270	23.423	.62
16	25.03	24.464	23.805	.566
17	25.21	24.700	24.194	.51
18	25.43	24.974	24.594	.456
19	25.69	25.280	25.	.41
20	25.96	25.615	25.414	.345
21	26.30	25.976	25.835	.324
22	26.65	26.360	26.265	.29
23	27.03	26.764	26.699	.266
24	27.43	27.186	27.149	.244
25	27.81	27.625	27.604	.185
26	28.20	28.076	28.066	.124
27	28.65	28.541	28.533	.109
28	29.08	29.017	29.016	.063
29	29.55	29.504	29.504	.046
30	30.	30	30.	----

The affinity of the experimental results to those derived from the new theory, is obvious upon inspection of the table; and the want of affinity to those derived from the old theory, is not less evident. The comparative relation of the two theories to the results of experiment, is more readily seen in the annexed cut (fig. 2), where they are respectively delineated by a curve. The upper curve represents the densities or elastic forces in the chamber, as found by experiment; the next curve those due to the new theory, and the lower curve those due to the old theory.

Notwithstanding the near approximation of the experimental

Fig. 2.



Numbers at top represent the densities in the receiver; those on the side, densities in the chamber.

results to those due to the new theory, there is yet a small but distinct deviation, which holds throughout. This deviation indicates either that there is some cause affecting the flow which the theory does not take into account, *or* that in the structure of the apparatus or in trying the experiment, there was some failure to comply with the requisite conditions.

Although the apparatus was rude in its structure, yet care had been taken to secure a compliance with the conditions on which the experiment was based; and in conducting the experiment I was assisted by my friend, Prof. A. C. Twining, a gentleman distinguished for his accuracy in such matters. The experiment, moreover, was several times repeated, with no important difference in the results. For these reasons, in seeking the cause of the deviation, my first inquiry was whether it might be attributed to a change in the ratio of elastic force to density; the theory being predicated upon the assumption that this ratio is constant. It has been ascertained by experiment, that when air is condensed and then suffered to lose the heat evolved by condensation, the ratio of its elastic force to its density will be diminished. Hence it is certain that a part or the whole, or possibly even more than the whole heat evolved by condensation will be required to prevent that ratio from being diminished. Still, however, it has generally been assumed by philosophers (I know not on what grounds) that if air is *suddenly* condensed, so as not to allow the heat evolved by condensation to escape, the ratio of elastic force to density will be *increased.* This assumption was made by Laplace when he attributed to this cause, in part, the velocity of sound. Let us suppose then, for the present, that in sudden condensation the ratio of elastic force to density is *increased.* It will then follow that in sudden *expansion*, the ratio of elastic force to density will be *diminished.* But if that ratio were diminished, then the deviation in the table should be in the opposite direction; that is, the experimental results, instead of being greater than the theoretical, should be less. The deviation, therefore, is not accounted for by this supposition; on the contrary, the experiment seems to prove that the ratio is not diminished by expansion, and therefore cannot be increased by condensation, as Laplace supposed.

Let us next take the contrary supposition, viz: that the ratio of elastic force to density is *increased* by expansion. This would cause a deviation in the same direction as we find in the table.

In order to ascertain whether the deviation in question is due to this cause, we must next inquire whether a deviation arising from this cause, would vary in the same manner throughout the table, as does the observed deviation. Now if we go through the table and assign for each observation severally, the manner in which the ratio of elastic force to density must increase, in order to satisfy that observation, we shall find very nearly one and the same increment of the ratio demanded for all the observations. Hence if we attribute the deviation to this cause we should be obliged to conclude that one and the same change in the ratio takes place, whether the expansion be greater or less. But such a conclusion is obviously inadmissible. We cannot, therefore, attribute the deviation in question to a change in that ratio, either by increase or diminution.

Nor can we ascribe the deviation to that which is the chief cause of deviation from theory in the case of the flow of *liquids*, viz: the contraction of the stream in passing an orifice. For if that cause operated, it would affect the flow in the same ratio in both orifices, and therefore would not, in this case, affect the indications of the mercurial columns. Moreover, I think it can be shown, from consideration *a priori*, that the cause which produces the contraction of the stream in liquids, could not operate to affect the flow of expansible fluids.

Having satisfied myself that the deviation was not due to the causes above named, my next inquiry was, whether a difference in the sizes of the orifices (hitherto assumed to be equal) would cause a deviation corresponding to that in the table. In examining this point, I found that the experimental results would be very nearly satisfied throughout the table, by the assumption that the area of the second orifice was less than that of the first, in about the ratio of .933 to 1. As the two orifices had been made as nearly equal as they could be by forcing the same steel plug through both, I was confident that, as originally formed, they could not differ to this extent. But it occurred to me that some accidental circumstance might have occurred to diminish the inner orifice, and I suspected that the workman, in handling the brass plate after the orifice was made, had got dirt into it, and had omitted to cleanse it before soldering on the outer plate. To ascertain whether such was the case, I divided the tube near the second orifice, and, upon examining it with a microscope, discovered that there was dirt adhering around its inner periphery sufficient, I think, to cause a diminution of its area to the extent above named. Unfortunately this discovery was made after the arrangements for trying the experiment had been removed; and I have not since found leisure to replace them and try the experiment anew. But for this accidental circumstance no doubt there would have been a still nearer approximation of the experimental results to those derived from the formula. The coincidence, however, is sufficiently near to establish the truth of the new theory, so far as respects those points of difference between the two theories specified in the first part of this article.

The fifth column of the table shows the several differences between the experimental results, and those due to the new theory. It will be noticed that these differences increase slightly between density 10 and 13 in the receiver, before they began to decrease. This, I think, indicates a slight obstruction to the flow through the second orifice, when the density in the receiver becomes equal or nearly equal to that of the effluent stream. This increment at its maximum amounts to .088, corresponding to the pressure of that portion of an inch of mercury, and is, I think, the measure of the obstruction or resistance due to that circumstance. If this view of the subject is correct, then there would have been a deviation to this extent in this part of the table, even if the orifices had been equal.

It is desirable that further experiments of this kind should be tried by those who have better means at command than I had to do justice to the subject. To such as may be disposed to undertake it, I would suggest that a perfect equality of the two orifices might be secured by interchanging the discs, varying the sizes of the orifices until they gave the same indications in both positions.

If, after thus securing the equality of the orifices, there should still be a deviation in that part of the table where the elastic force in the chamber is constant, such deviation, I think, must be attributed to a change in the ratio of elastic force to density; and if so, its amount would furnish the means of determining the law according to which that ratio varies.

I would also suggest that a modification of this experiment would furnish perhaps the best possible means of determining the law according to which the ratio of elastic force to *temperature* varies, when the absolute amount of heat is constant. In an arrangement for this purpose, the bulb of a thermometer should be inserted into the chamber, and the outer orifice should be so constructed that it may be enlarged or diminished at pleasure. With this arrangement, we may cause the air in the chamber to assume, almost instantly, any elastic force we may choose, between the elastic force of the atmosphere, and a little more than twice the elastic force in the receiver; and we may keep that force *constant* in the chamber during any time that may be required to cool the thermometer down to the corresponding temperature, the continual flow through the chamber in the meantime carrying off not only the heat which flows in from extraneous sources, but also that derived from the thermometer itself. We may thus ascertain the relation of elastic force to temperature at as many points as we please within this range, and thereby determine the law of their variation when the absolute amount of heat remains constant.

ARTICLE V.

---

*The Form, Formation and Movement of Sonorous Waves.**

There can be no doubt that the movements of aerial particles resulting from forces impressed upon them, and the transmission of force from one to another are in perfect accordance with Dynamic laws. But the various attempts which have been made to determine by the application of these laws the precise order and extent of their respective movements have not been successful. While by the application of Dynamic laws to other matter we are able to trace with the utmost precision the paths of bodies in the remotest regions of space, we have as yet no definite knowledge respecting the movements of the air which surrounds us;—not even of those movements on which we are constantly dependent for the transmission to each other of our oral communications. There are many problems pertaining to this department of Physics the solution of which would be of great interest to science, but to solve which no attempts have as yet been made because of the apparent difficulties involved in the investigation. Whatever of thought has been bestowed in this field of inquiry has been devoted chiefly or wholly to the determination of the velocity of sound. But the attempts to solve even this problem, initiated by Newton, have been attended with so little success that now, after the lapse of two centuries, during which continual efforts have been made to reconcile the widely differing results of theory and experiment, scientists are not agreed whether this, or that, or both, require the correction which should bring them into harmony.

* Read at a meeting of the Connecticut Academy of Arts and Sciences, December 21, 1881.

The delay to develop this branch of Physics has been due, no doubt, to the peculiar and complex nature of aerial matter; its tenuity, its fluidity, its compressibility and its elasticity. These properties have seemed to scientists to conspire to embarrass the application of Dynamic laws to the air; while its total invisibility has compelled them to pursue their investigations in the dark, by abstract thought alone, and without the aid of the senses to check the aberrations of reason.

On account of these properties of aerial matter the problem to find the velocity of sound has always been regarded by mathematicians as one of great difficulty. The late Professor Peirce, in his Treatise on Sound, says, "The problem to investigate the general laws of the propagation of sound is one of the utmost complexity, and has been resolved only under very restricted conditions." After alluding to the profound researches which have been bestowed on this subject by Euler, Lagrange and others, he proceeds to investigate the problem restricted to the case of propagation in a tube of uniform calibre. After constructing an elaborate series of differential equations to this end, he at length arrives at one which he says "is altogether intractable and incapable of integration." In order to proceed further he finds it necessary to restrict the conditions of the problem still further by confining it to the case in which the elastic force of the wave exceeds that of the medium in which it is propagated by an infinitesimal quantity only; that is, by confining it to a sound of infinitesimal intensity. With the problem thus restricted he goes on, and after a very elaborate process, arrives at the conclusion that the velocity of sound is that which a heavy body will acquire by falling through half the height of a homogeneous atmosphere. This is the same conclusion as that arrived at by Newton, Euler, D. Bernouilli, Lagrange, Poisson, Laplace, and I know not by how many others, all of whom found it necessary to restrict the problem to like conditions.

When nature is questioned respecting her laws, even by the most profound mathematicians and by the most recondite processes of analysis, if those processes are based upon assumptions not consonant with her laws she will refuse to respond except in equations "intractable and incapable of integration." It is the purpose of this communication to show that those who have attempted to solve this problem have made an erroneous assumption respecting the manner in which an impinging force imparts motion to aerial matter, and that upon a correction of this error the difficulties which they encountered in their attempts to give a *general* solution to the problem will vanish, and we shall be able to arrive at such a solution in a direct and simple way.

The mistake to which I have referred consists in having assumed that motion is imparted by a finite force to the *infinitesimal particles* of aerial matter in the same manner as to *finite masses* of matter; that is by *acceleration:* or, in other words, that in the former as well as in the latter case motion begins with an infinitesimal velocity which is augmented to a finite velocity by infinitesimal increments. Such a notion is not consistent with Dynamic laws. These laws demand that when a force of finite magnitude acts upon an infinitesimal quantity of matter a finite velocity proportionate to the force should be imparted in an infinitesimal time. An impinging force therefore does not impart motion to aerial matter by acceleration.

But (it may be asked here), has not Newton in the 47th proposition of the Second Book of the Principia, demonstrated that each infinitesimal particle of air that is put in motion by a sonorous wave is accelerated and retarded according to the law of the vibrating pendulum?

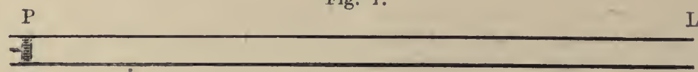
It is true that the proposition referred to *purports* to demonstrate this; but a careful examination will show that it fails to do it. In this proposition Newton at the outset assumes in his hypothesis the point to be proved and the conclusion at which he arrives results from that assumption. It is in short a notable example of "reasoning in a circle." This will be evident upon a close scrutiny of the argument: and it is also clearly shown by Gabriel Cramer* who, to prove the inconclusive character of Newton's reasoning in this proposition, shows that by a precisely similar course of reasoning (*et, mutatis mutandis, in totidem verbis*) it may be made to appear that the law of acceleration is that which pertains to a uniform force, as in the case of bodies falling by their own gravity.

We may conclude then that it has not been demonstrated by Newton that the particles of air put in motion by a sonorous wave acquire their velocity by acceleration; and therefore we may accept the conclusion to which we were led by Dynamic laws, that each particle as successively encountered receives its full velocity instantaneously.

* See Principia, Glasgow Edition, page 273, note.

Let us now consider the *modus operandi* by which the force so imparted is passed along from particle to particle; and to facilitate our conceptions of the process let us suppose it to take place in a tube of uniform calibre. Let PL, Fig. 1 be such a tube, and let P be a piston fitted to it.

Fig. 1.



If a uniform velocity v be instantaneously imparted to the piston, the plate of air which is in contact with its front will instantaneously receive the velocity of the piston and at the same time be condensed by a force equal to that required to overcome the inertia of the plate. Suppose the velocity v to be such that this condensing force is sufficient to reduce the thickness of the plate $\frac{1}{10}$; then will this plate, thus reduced in thickness, be added on to the front of the piston and go on with it, condensing the next plate, giving it the velocity v and pushing it on in front of itself, and so on. Thus it will be seen that when the condensing force is that here supposed, the point where the condensation takes place moves forward in the tube just ten times as fast as the piston. When therefore the piston has moved $\frac{1}{10}$ of an inch there will be $\frac{9}{10}$ of an inch of condensed air in front of it moving with the same velocity as the piston. If now, at this juncture, the motion of the piston be arrested, the condensed air in front of it will continue to move on by its momentum condensing and adding plates to its front as before, and in the meantime the rear end of this condensed air having advanced beyond the piston will have space to expand and resume its normal volume and come to rest: and as it respects each infinitesimal plate, the restoration will be as instantaneous as was its condensation. That such will be the manner of restoration need not be shown here, since it will fully appear from what is shown in an article on the mode of Expansion of Elastic Fluids in the American Journal of Science, second series, vol. ix, page 334.* Thus we have a self-sustaining, self-propagating wave, the quantity condensed in front in a given time being just equal to the quantity expanded in the rear in the same time; the latter by its reaction furnishing the power to keep up the motion and condense the plates in front.

* The *third* of the preceding articles.

A *wave* then, in the sense in which we shall employ the term in this paper, consists of a quantity of air of a uniform density greater than that of the medium in which the wave is propagated, and having the absolute velocity that is due to the action of the force which effected its increased condensation.

It may be well here to define a few other terms or phrases, some of which we may have occasion to employ in a sense somewhat different from that attached to them by other writers on this subject.

The *breadth of a wave* is the space occupied by the condensed air measured in the direction in which it moves.

The *intensity of a wave* is the excess of its elastic force or density over that of the medium.

The *velocity of a wave* is the velocity of the point where the particles are condensed and put in motion.

The *velocity of the particles* is the absolute velocity instantaneously impressed upon the particles successively by the wave.

In the use of the term *particles* we are not to be understood as indicating any theory respecting the constitution of the atmosphere,—we mean small parts or portions,—not molecules or atoms.

In showing how a wave may be formed in a tube we have supposed the motion of the piston to be arrested after moving $\frac{1}{10}$ of an inch. If it had been arrested after moving any smaller distance the *breadth* of the wave would have been smaller in the same ratio, but in every other respect it would have been the same; it would have had the same intensity, the same velocity, and it would have imparted the same velocity to the particles; these quantities being dependent only upon the velocity of the piston, and not on the distance through which it moved before its motion was arrested.

We have thus shown how a self-propagating wave may be formed in a *tube*. The process of the formation of such a wave in the open air is, in principle, precisely the same, and the form of the wave and laws of its propagation are the same.

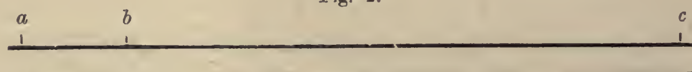
It is well-known that sounds of small intensity propagated through tubes have the range of their audibility extended to a greater distance than it could reach if propagated in the open air; but this function of the tube does not involve any modification of the laws of propagation on which the velocity of the wave depends. The reason why a tube has this effect may be readily illustrated. Suppose we present the end of a tube to a vibrating body or other agent that may originate a wave; then a portion of the condensed air which constitutes the wave at its origin will enter the tube, constituting a self-propagating wave therein; the rest of it will spread out forming a spherical shell or wave whose thickness measured on the radius of the sphere is the breadth of the wave; and since there can be no increase in the quantity of this condensed air after the wave is formed, the thickness of the shell or breadth of the wave will be inversely as the square of the distance from the place of its origin. Thus we see that the breadth of the wave which is propagated outside of the tube diminishes in a rapid ratio; but the breadth of that which is propagated inside of the tube undergoes no diminution except such as may be due to friction or imperfect elasticity. Now it is evident that, other things being equal, the audibility of a sound must be as the breadth of the wave; and that a sufficient breadth of wave may be maintained in the tube after the breadth of that outside shall have been so far reduced as to be incapable of producing the required action upon the acoustic organs. This is the only difference there is between waves propagated in the open air and those which are propagated in a tube, and as we have shown, it is not a difference that affects the laws of their propagation.

From the law of the transmission of force from particle to particle, as we have shown it to be, we will proceed to deduce the law of the propagation of sonorous waves.

Prop. I. The velocity of a wave of whatever intensity is to the absolute velocity which it impresses instantaneously on every particle over which it passes, as the space occupied by the condensed air before condensation to its loss of space by condensation.

The truth of this proposition may be drawn as a corollary from what was shown in describing the formation of a wave in a tube, and therefore we need to give no further proof of it here.

Fig. 2.



Let *bc* fig. 2 be a column of air of the density and elastic force due to the pressure of the atmosphere, and whose length is equal to the height of a homogeneous atmosphere. Let this column be extended to any distance *a*; and let a wave whose elastic force exceeds the pressure of the atmosphere in the same ratio as *ac* exceeds *bc* pass over the column *ac*.

Prop. II. The velocity of a wave like that above described will be to the velocity which it impresses on the particles over which it passes as *ac* to *ab*.

For it is evident that such a wave will condense the particles in the ratio of *ac* to *bc*, and therefore the space occupied by the condensed particles before condensation will be to their loss of space by condensation as *ac* to *ab*, and therefore by Prop. I the velocity of the wave is to that which it impresses on the particles as *ac* to *ab*.

Prop. III. The velocity impressed upon the particles is that which a falling body would acquire in the time in which the wave passes over *ab*.

For during that time the excess of elastic force which is a constant force and which is equal to the weight of the air in *ab* is acting upon that air, and therefore must impart to it the same velocity as it would acquire by falling during the same time; and that velocity is the same as would have been acquired in the same time by any other falling body.

Prop. IV. The velocity of the wave is that which a falling body will acquire in the time in which the wave runs over *ac*.

For it was shown in Prop. III that in the time of running over *ab* a falling body would acquire the velocity which the wave impresses upon the particles, and since by Prop. II that velocity is to the velocity of the wave as *ab* to *ac*, it follows that in the time of running over *ac* the falling body will acquire the velocity of the wave.

Prop. V. The velocity of a wave is that which a body will acquire by falling through a height which exceeds half the height of a homogeneous atmosphere in the same ratio as the elastic force of the wave exceeds the elastic force of the medium in which the wave is propagated.

It was shown in the last proposition that in the time in which the wave runs over *ac* a falling body would acquire the velocity of the wave, and since the mean velocity of falling bodies is half their final velocity, it follows that in the time in which the wave runs over *ac*, the falling body in acquiring the velocity of the wave, will have fallen through a space equal to half *a c*; and since *b c* is by construction equal to the height of a homogeneous atmosphere, and since *a c* exceeds *b c* in the ratio in which the elastic force of the wave exceeds that of the air, it follows that half of *a c* exceeds half the height of a homogeneous atmosphere in the same ratio. Therefore the velocity of a sonorous wave is that which a body will acquire in falling through a height which exceeds half the height of a homogeneous atmosphere in the same ratio as that in which the elastic force of the wave exceeds the elastic force of the medium in which the wave is propagated.

Thus we have a *general* solution of the problem relating to the propagation of sound--a solution limited by no conditions. From this solution *it appears that an excess of elastic force above that of the medium is essential to the existence of a wave ; and also that the greater this excess, the greater will be the velocity of the wave.*

If we apply this *general* solution to the particular case of a wave whose intensity (or excess of elastic force), is so small that its effect on the velocity is inappreciable, we shall find the velocity of such a wave to be that which is due to half the height of a homogeneous atmosphere. This is the velocity found by Newton and those who succeeded him in this investigation, all of whom, in consequence of their assumption of the gradual acceleration of the particles, found it necessary to confine their investigations to waves of this small intensity. It seems obvious that a result thus obtained can be legitimately applied only to such waves ; but we find that these scientists have regarded and treated it as equally applicable to all waves. They maintain that the velocity which a body acquires by falling through half the height of a homogeneous atmosphere is the theoretical velocity of *all* waves so far as their velocity depends on the laws of Dynamics.

I think it may be shown how those scientists have been led to this conclusion as a consequence of their acceptance of the theory of gradual acceleration. It is evident that in a wave originated and propagated by gradual acceleration (if such a wave can exist, which I do not admit), the foremost particles can have only an infinitesimal condensation, however dense the particles may be furthest back. Now it appears both from the general solution we have here given and from the more limited one given by others, that particles of so small condensation can only transmit their force from one to another with the velocity due to half the height of a homogeneous atmosphere; and since a wave can move no faster than its foremost particles it would thence follow that no wave would move faster than with the velocity due to half the height of a homogeneous atmosphere, and consequently that all waves have that velocity.

We see then that if we adopt the theory of gradual acceleration we cannot avoid the conclusion that all sounds have the same velocity. But we shall see how absurd are the results to which we should be driven by accepting the conclusion that all waves have the same velocity.

According to Professor Peirce the theoretical velocity of sound is 916 feet per second, and its actual or observed velocity is 1090 feet per second (Peirce on Sound, page 31). The great difference between these velocities was a mystery to mathematicians from the time of Newton to that of Laplace. Laplace suggested that the difference was due to the heat evolved by condensation. This suggestion, coming from so high a source, has been accepted by most mathematicians without examination as an adequate supplement to the theory of Newton, and sufficient to reconcile the results of theory with those of observation. Others who have investigated the question have doubted its sufficiency for that purpose. Professor Peirce, after remarking that the heat evolved by sudden condensation *may* be much greater than we should expect in the case of such small condensations as are contemplated in the theory of sound, accepts with manifest misgivings and reluctance the sufficiency of Laplace's explanation, and employs it to bridge over this otherwise impassable gulf in the theory of the propagation of sound.

Now let us see how much the temperature of a wave must be raised by evolved heat to increase its velocity from 916 to 1090 feet per second. According to Professor Peirce the velocity of sound increases 0·96 feet per second for every degree of temperature above 32° Fahrenheit. Then by the formula

$$\frac{1090-916}{0·96}=181\tfrac{25}{100},$$

we find that 181.25 degrees is the elevation of temperature required to increase the velocity of a wave from 916 to 1090 feet per second. And since under the theory of gradual acceleration the front of every wave must consist of particles only infinitesimally condensed, and since this part must have its velocity increased as much as any other part, it would follow that an infinitesimal condensation would cause an evolution of heat that would raise the temperature 181 degrees! It would further follow that we could not converse with a friend, even in the mildest whispers, without pouring into his ears waves of a temperature of 181+32=213 degrees, or hotter than boiling water!! Moreover it would also follow that upon the slightest sudden change of barometric pressure we should be immersed in air of a temperature of 213 degrees!!

Such are some of the absurd conclusions which follow from the doctrine of gradual acceleration supplemented by evolved heat.

The doctrine that the particles which constitute the wave have all their motion impressed upon them instantaneously, leads to no such absurd conclusions and presents no such hiatus to be bridged over by evolved heat or otherwise; as we will now attempt to show.

It follows from our solution of the problem of the propagation of sound that the *intensity* of the wave (an item which was eliminated in the old theory), is a quantity not to be disregarded in determining the velocity of sonorous waves. Let us see if this item, when given its proper place in the investigation, is not sufficient to effect a reconciliation between the results of theory and those of observation; and that without calling in the aid of evolved heat.

Prof. Peirce gives 916 feet per second as the *theoretic* and 1090 as the *observed* velocity of sound. But we should here note the significant fact that 916 is the computed velocity of a wave so weak that its intensity may be regarded as cipher; while 1090 is the observed velocity of the report of a cannon, one of the most intense sounds known to us. According to both the old and the new theory, 916 is the *theoretic* velocity of the weaker of these waves. If then we can show that under the new theory 1090 is also the theoretic velocity of the report of a cannon, we shall have effected a full reconciliation between theory and observation without calling in the aid of evolved heat. In order to do this the question to be solved is this,—is the intensity of the wave produced by the discharge of a cannon sufficient to account for the difference between 916, the velocity of a wave whose intensity is regarded as cipher, and 1090, the observed velocity of the report of a cannon. If we knew what is the intensity of the wave so produced we could readily ascertain whether it is sufficient for this purpose; but as we have no means of learning this, let us inquire what the intensity of such a wave must be in order that its velocity may be 1090 feet per second. Referring to fig. 2 where *bc* represents the height and density of a homogeneous atmosphere and *ab* the intensity of the wave, the question to be solved is, what must be the ratio of *ab* to *bc* when the velocity due to half the height *bc* is 916 feet, and that due to half the height *ac* is 1090 feet? By an obvious process, which need not be given in detail here, it will be found that *ab* must in such case be about $\frac{41}{100}$ of *bc*; that is, the density of the wave must exceed the density of the air by about 41 per cent.

That the density of the air in front of the muzzle of a cannon is upon its discharge increased as much as 41 per cent., and often very much more than 41 per cent., there can be no doubt. It is evident that the degree of that condensation is not always the same, but must depend on various considerations, such as whether the cannon be charged with shot,—whether the wad be so compressed as to require great force to drive it out,—the length of the piece,—the quantity and quality of the powder, &c. In the records of the various velocities observed no reference is made to these considerations. Prof. Peirce gives 1090 as the mean of seven selected observations, none of which varied materially from 1090. But there are many other observations recorded giving observed velocities much higher than those which he selected. These are rejected by Prof. Peirce as unreliable; for what reason is not apparent. For my own part I doubt not that waves are often produced by the discharge of cannon whose intensity is such that under our theory their velocity of propagation would far exceed 1090 feet per second. If this be so, we shall need no aid from evolved heat until, in the case of any particular wave it can be shown that its velocity is greater than under our theory can be due to its intensity. It may be asked here whether I reject the notion that the velocity of sound is increased by evolved heat? I answer that I do not; but I think that under the old theory the effect of heat has been greatly overrated. I think its effect must be in proportion to the quantity of heat evolved, and that the quantity evolved must be in proportion to the degree of condensation. I think therefore that when the condensation is infinitesimal the effect of the evolved heat will be inappreciable. If this be so, then, whenever a method shall be devised whereby we may find by experiment the velocity of waves of infinitesimal condensation, we shall find it not to differ materially from that which a body will acquire by falling through half the height of a homogeneous atmosphere; —the velocity found by Newton.

Those who have investigated the propagation of sound under the old theory seem to have supposed that the evolved heat has a *thermo-dynamical* effect; that it is converted into mechanical power and so acts *mechanically* to increase the velocity of sound; but it can have no such action, for the supposed force of the heat is expended within and upon the very matter which is supposed to be propelled by it, and therefore acts backward as much as forward. A vessel sails no faster because she is on fire in her hold; the earth revolves no faster upon its axis because of a fire on its surface or within it; the heat of the fire under the boiler, or of the steam within it does nothing to propel a steamer, until, through the intervention of machinery it is made to *react* upon matter *without*. Archimedes could move the world, but he must have something outside of the world for his lever to react upon. In the case of the heated wave there is nothing outside of the wave for the heat to react upon, and therefore it can have no *mechanical* effect to increase its velocity. The mechanical power of a wave to perform the work of reproducing itself is given to it by the power which originates the wave, and it can neither be increased or diminished by the heat evolved by condensation. What then is the *modus operandi* in which the velocity of a wave is increased by evolved heat? It is simply by increasing the ratio of the elastic force of the wave to the density of the medium in which it is propagated. The elastic force of the *wave* is increased while the density of the *medium* is unchanged. The result of this change of ratio is that the velocity imparted to the particles by the wave is *diminished*, while the number of particles acted upon in a given time is *increased* in the same ratio, so that the amount of *mechanical work* performed is neither increased or diminished by the evolved heat.

In the book published a few years since, entitled, "The Correlation of Forces" it was stated that the eminent German scientist Mayer had deduced the mechanical equivalent of heat from the velocity of sound; and with a result coinciding very nearly with the equivalent found in other ways. This statement was apparently regarded by the author as being highly confirmatory of the doctrine of his book. This computation by Mayer, however, could only be founded on the assumption that heat acts *mechanically* in increasing the velocity of sound; and therefore it is evident from what has been shown that whatever other ground the doctrine of the correlation of forces may have to rest upon, it can derive no legitimate support from this computation by Mayer. We have another example of a similar mistake in the recent announcement that another scientist has deduced the velocity of sound from the mechanical equivalent of heat.

I had intended to present here several other interesting conclusions at which I had arrived on this subject, but I forbear, lest I should extend this paper to too great length; and I pass them by with the less reluctance, because any one who will be likely to take the trouble to read this paper will be able himself to arrive at the same results.

In conclusion I think proper to mention the fact that more than thirty years since I contributed to the Journal of Science an article* in which I demonstrated in a manner quite different from that pursued in this article, but not less conclusive, that all waves have not the same velocity, but that their velocities vary with their intensity; and that the velocity of a wave was that which a body would acquire by falling through a height greater than half the height of a homogeneous atmosphere in the same ratio as the density of the wave exceeds the density of the medium in which it is propagated:—results precisely similar to those deduced in this article.

That article seems to have attracted little or no attention: for up to the present moment all scientific periodicals continue to speak of the velocity of sound as if it were a quantity determinate and invariable, sought for, but not yet ascertained with precision and certainty; and scientists, by ingenious theoretical and experimental devices still continue to search for the mythical number as the Alchemists did for the Philosopher's stone. Possibly this article may have as little effect as the former to bring those labors to an early close; but however this may be I cannot doubt that sooner or later the fact will be recognized and accepted, that sound waves do not all move with one and the same velocity; and that the difference in their velocities is due chiefly to the difference in the intensities of the forces by which

* Journal of Science, second series, vol. v, page 372. The second of the preceding articles.

they are respectively originated, and only in a much smaller degree to the elevation of the temperature of the wave by compression : and it will be seen that we have no need to resort to the theory of Laplace to account for the difference between the volocity of the wave formed by the report of a cannon and that of a wave of infinitesimal intensity.

Answers to the principal objections which have been made to the last of the foregoing Articles.

Objection I.

In the foregoing articles it is maintained that sound waves which differ in intensity are not propagated with the same velocity. To this doctrine it has been objected that when a tune is played at a great distance from the hearer, the notes reach his ear in regular order and in their proper sequence in respect to time; which, it is alleged, would not be the case if the waves moved with different velocities. Let us inquire into the force of this objection.

If we consider the minute spaces through which strings, reeds, etc., vibrate in giving origin to musical sounds, and the limited number of vibrations made per second, whatever may be the pitch, we shall not be able to make out that the velocity of the vibrating string or reed is in any case greater than one or two feet per second. In the computation upon which we are entering, the greater we assume that velocity to be the more will the result favor the objection; and as we can afford to be very liberal to the objector we will assume that the greatest velocity that can pertain to any vibrating body that sends forth musical notes, *may* be 10 feet per second. The vibrating body imparts to the air its own velocity; and, from what is shown in Article V., it follows that in putting air in motion with a velocity of 10 feet per second, its density will be increased only so far as to exceed the normal density by about one per cent. Now suppose that in the weakest waves that can transmit an audible sound there is a condensation of one-tenth as much: then the densities of the strongest and weakest sonorous waves that can be produced by musical instruments will be to each other as 101 to 100·1, and their velocities of propagation as $\sqrt{101}$ to $\sqrt{100 \cdot 1}$. Consequently, if two waves, thus differing in density, be originated simultaneously at a distance of 1000 feet from the observer, while the denser wave runs over the 1000 feet, the less dense wave will run over $\frac{\sqrt{100{\cdot}1}\times 1000}{\sqrt{101}}=995{\cdot}534$ feet, and will then be behind the other $1000-995{\cdot}534=4{\cdot}466$ feet in *distance*; and in *time* (if the velocity of the denser wave is 1000 feet per second), $\frac{1}{995{\cdot}534\div 4{\cdot}466}=\frac{1}{223}$ of a second.

Thus it appears that two waves having as great a difference of intensity as can exist between any two waves produced by musical instruments, if originated simultaneously at the same place and thence propagated 1000 feet, will then be separated by 4·466 feet in space and by $\frac{1}{223}$ of a second in time.

But the impression of a musical note on the ear is not produced by a *single wave*, but by a series or group of waves; occupying in their production seldom or never less than $\frac{1}{20}$ of a second, and therefore extending through a space of $\frac{1000}{20}=50$ feet. Consequently the two groups of waves, of which the single waves we have been considering respectively constitute the first, overlap each other and are coincident for more than $\frac{9}{10}$ of their length.

That displacements as great as that above considered do not perceptibly impair the regularity and harmony of a tune is evident from the fact that in every musical performance however perfect, there are constantly recurring deviations from the true time of the tune to the extent of not less than $\frac{1}{223}$ of a second; and such deviations must produce displacements of like extent, even though we assume that the velocities of all waves are precisely equal.

Again—in the case of a number of musicians performing in concert, although we should assume that all waves move with the same velocity and that there is not the slightest deviation from the true time, yet if the difference in the distances of the hearer from the respective performers should amount to 4·466 feet—say 4½ feet,—there would result an equal displacement of the notes. I think it will not be claimed that the most fastidious critic of musical performances could perceive a distortion of the music in such a case.

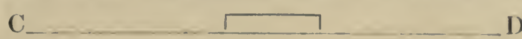
Our conclusion then, is that the apparent regularity in the order and chronic sequence of the notes of a tune, performed at any distance from which it can be heard, does not go to show that all sound waves are propagated with the same velocity.

Objection II.

Those who have heretofore attempted to investigate the law of the propagation of sound have assumed that a sonorous wave consists of contiguous particles of compressed air whose increased density is greatest at or near the centre of the wave, diminishing thence to cipher at the front and rear, and whose particles respectively have the velocities due to the respective forces by which they have been compressed; and they have illustrated their view of the nature and constitution of a wave by a diagram constructed substantially as follows; where AB is a line of particles over which

a wave is passing from A to B; *cd* is the breadth of the wave; and an ordinate from the curve above to any point in *cd* represents the compressing force and velocity of the particle at that point.

In the fifth of the preceding articles a sonorous wave is defined as consisting of contiguous particles of air, all equally compressed and having the same velocity. Such a wave, shown by a diagram constructed on the same plan as before, would show the wave in the form presented on the line CD; with a horizontal line above and precipitous termini in front and rear.



According to the view of a wave as presented on AB, the quiescent particles of air as they are successively encountered by the advancing wave commence their motion with an infinitesimal velocity which is augmented by insensible degrees, reaching its maximum at the centre of the wave; but a wave of such form as is shown on CD requires the full maximum velocity to be imparted to the quiescent particles instantaneously.

To this last view of a wave the objection is made that a finite velocity cannot be impressed upon matter *instantaneously*.

In making answer to this objection it is proper to say at the outset that the term *instantaneously*, as used in the foregoing articles, does not import an absolute *negation* of time. It is employed in its more common sense, to indicate a time shorter than any assignable time. Thus understood the view objected to is in perfect harmony with that universal law of dynamics,

$$FT \propto MV$$

or, The product of force and time is as the product of mass and velocity.

For, let V represent the absolute velocity imparted to the particles of air encountered by the wave; let F represent the constant force required to overcome the inertia of those particles in being put in motion with the velocity V; and let M be the mass of air put in motion with that velocity in any time, T. Then F will be as V, and M will be as T, and consequently if we make M infinitesimal, T will be an infinitesimal of the same order. We have then, a finite velocity, V, imparted by a finite force, F, to an infinitesimal mass, M, in an infinitesimal time, T; thus sustaining the view which was objected to. In this case I regard the infinitesimal mass as taking the velocity V, without first passing through the smaller velocities that may be assigned between V and cipher; and I know of no law of nature which precludes the assumption that such is the fact. If, however, the objector thinks that even in this case the velocity V, is acquired by gradual acceleration and can conceive how this gradual process can be carried through in an infinitesimal time of the highest order and within the limits of a space equally minute, such a view will not conflict with the views maintained in the fifth article nor call for any modification of the form of the wave as presented on the line CD.

Objection III.

It has been objected to that part of the fifth article where I have estimated the extent to which the temperature of a wave must be raised by evolved heat, in order that its velocity may be increased from 916 to 1090 feet per second, that I have assumed that an elevation of the temperature of the wave one degree by evolved heat will have no more effect to increase the velocity of the wave than an elevation of the temperature of the medium one degree; it being alleged by the objector that one degree of heat, confined to, and acting wholly within the wave itself must have a vastly greater effect to increase the velocity of the wave than the same amount of heat diffused throughout the medium.

This objection seems to ignore the distinction between *degree* of heat and *quantity* of heat: but I forbear to assume that the objector has overlooked a distinction so wide and so obvious.

Other things being equal, the velocity of a wave is directly as the square root of its elastic force and inversely as the square root of the specific gravity of the medium in which it is propagated. An elevation of temperature, whether of the wave or of the medium, has no effect to increase the velocity of the wave, except so far as it increases the ratio of the elastic force of the wave to the specific gravity of the medium. To elevate the temperature of the *wave* increases that ratio by increasing the elastic force of the wave. To elevate the temperature of the *medium*, increases that ratio by decreasing the specific gravity of the medium. Hence the question raised by the objector is whether equal increments of temperature in the two cases result in equal increments of that ratio. I maintain that the effects of the elevation of temperature are precisely the same in both cases to increase that ratio, and to increase the velocity of the wave. For an elevation of the temperature of the wave one degree, by increasing the elastic force, gives to the air of the wave a tendency to expand and reduce its specific gravity in the same ratio that the heat had increased its elastic force. An elevation of the temperature of the medium one degree causes it to expand and reduce its specific gravity in that same ratio. The effect therefore upon the velocity of the wave is the same in both cases.